

Contemporary Psychology

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Learning Theory—End of an Era?

William K. Estes, Sigmund Koch, Kenneth MacCorquodale, Paul E. Meehl, Conrad G. Mueller, Jr., William N. Schoenfeld, William S. Verplanck

Modern Learning Theory: A Critical Analysis of Five Examples
New York: Appleton-Century-Crofts, 1954. Pp. xv + 379. \$5.00.

Koch's Analysis of Hull

By FRANK A. LOGAN

Yale University

SIGMUND KOCH recognizes Hull's many contributions to psychology. He holds that "Hull unquestionably comes out as top man . . . among the influential learning theorists . . . with respect to explicitness of axiomatization" (p. 56) and applauds Hull's attempts "to bring the principles to bear in a relatively detailed way on experimental issues" (p. 86). He doubts that "competing systematists have yielded more gallantly to the encroachment of fact" (p. 98) and suggests that Hull's theoretical intentions led "to a marked improvement in the quality of experimental designs" (p. 166). In general, Koch is probably correct in saying that "from 1943 to the present, [Hull's] conceptions have literally dominated the scene within 'learning theory', either as foci of controversy, or as sources

of hypotheses for further experimental elaboration and evaluation" (p. 102).

Paradoxically, however, in reading Koch's analysis of *Principles of Behavior*, one searches almost in vain for a concept found to be adequately defined, for a construct held to be securely anchored, for a construct interconnection viewed as determinant, for a postulate thought to be properly constructed, for a theorem seen to be meaningfully derived, or, in general, for any theoretical contribution left unscathed. According to Koch, "close analysis brings so many dimensions of ambiguity into view that the apparently crisp contours of each postulate, and the set as a whole, fades away into uncertainty" (p. 60).

This paradox of a highly useful theory appearing under systematic analysis to be so useless seems not to be adequately resolved by Koch, and this reviewer can but suggest some of the apparent reasons for this inconsistency. Most fundamental is the fact that Koch disagrees with the



CLARK L. HULL

general approach to theory construction which Hull employed. Hull used the techniques of the highly successful physical sciences as a model, believing that a hypothetico-deductive behavior theory, stated in exact quantitative form, might ultimately be achieved. He believed further that the most efficient way to attain this goal is to use tentative approximations to an ultimate theory as a frame of reference while working toward more rigorous formulation, more exact quantification, and more general applicability.

Koch does not share Hull's beliefs. Instead, he thinks that there may be "limits, in principle, on realizing hypo-

thetico-deductive systems" (p. 57), that realizing such a system is "enormously out of reach" (p. 77), and that attempts such as Hull's are "doomed to failure" (p. 71). Indeed, according to Koch, "In the present state of our ignorance, no one can seriously believe that a comprehensive, quantitative, hypothetico-deductive theory of behavior is possible" (p. 159), and Hull's view "can well block what chances we have of moving towards adequate theoretical formulations of behavior" (p. 161).

Koch does not object simply to *comprehensive* theories, for he argues that the "concrete problems" that caused Hull's "inevitable failure" "remain equally unresolved for 'theories' of all sizes; the scope of such problems does not automatically scale down to the scope of the theorist's intentions" (p. 161). It is also unlikely that Koch objects solely to *quantification*, for correctly he views quantitativeness as a continuum (p. 65). Koch, therefore, apparently feels that the *hypothetico-deductive* form of theory is not applicable to the behavioral sciences.

This is a more basic disagreement than one over the contents of particular postulates, for it concerns the general model of theory construction, the theory of theories. Koch does not, however, describe what other form of model he favors, and it is therefore difficult to know exactly what characteristics of Hull's model Koch finds objectionable. The intended constructive purposes of his analysis would have been better served had he described his positive beliefs. He does, indeed, imply that alternative models may be found in other sciences, or may yet have to be devised; this possibility cannot be denied; but, while it would be foolish to permit Hull's opinion to blind one to the possibility of more adequate approaches, it would be even more foolish to abandon Hull's approach with no alternative and for no reason better than that it *might* not be the best.

THERE are many points in Koch's paper which, while often worded in an unnecessarily emotional manner, state genuine difficulties with Hull's theory as it is currently formulated. For example, Koch notes correctly that, "*In Principles of Behavior, no formal operational definitions are given*" (p. 18). This is, formally,

a serious omission, and Koch is probably correct in remarking that a preliminary attempt at such definitions "might either have exposed many . . . ambiguities, or eliminated them" (p. 59). Nevertheless it is also proper to inquire how serious this omission is in actual practice.

CONSIDER, for example, the problem of defining the term *stimulus*. Hull at times has stated that stimuli are to be "specified in terms of *independent physical energy criteria*" (p. 22). Elsewhere, Hull has implied that stimuli are "identified by reference to responses . . . as any part of the environment, discriminated by the experimenter, which impinges on the organism simultaneously with the occurrence of R" (p. 22) and which "bears a demonstrable lawful relation to R" (p. 25). Koch finds in these two statements a "systematic ambiguity" (p. 23), reflecting "one of the most fundamental inadequacies of the theory" (p. 42). Surely the first tentative approximations of a theory deserve a gracious, and at times generous, reading. Actually, Hull's second expression should be taken, not as inconsistent with, but as an elaboration of, the first. Science requires abstracting from an infinity of possible physical energies which could be "specified," and therefore some conventions must be adopted to "identify" the presumably relevant energies. While such a procedure is not wholly adequate as a definition of a stimulus, especially if one is dealing with problems such as perception, Hull's statements permit experimenters to circumvent the absence of a formal definition and to agree that some events (e.g., a buzzer) are stimuli in the sense of the theory.

Consider another one of Koch's basic objections to Hull's methodology. His key points may be abstracted as follows: "two extremes of what is perhaps a continuum of possible techniques" of theory construction are the "rational" and the "empirical" (pp. 66). Since, however, "there can be no direct parallelism between [a general intervening variable function] . . . and any experimental 'realization' of that function" (p. 73), the empirical techniques are not useful in building comprehensive theories. Koch, therefore, repeatedly arraigns Hull for "this practice of constructing intervening-variable functions directly from empirical

materials" (p. 79) and then uses this charge as one basis for concluding that Hull's postulates "cannot possibly be empirically valid over the very general (indeed universal) range of reference for which they are asserted" (p. 92).

Koch's analysis and the resulting evaluation are not, however, entirely correct. Instead, it seems more likely that postulates whose elements are not at the theorist's immediate observation level are *always* constructed as "rational" guesses. When confronted with a large number of possible guesses, a theorist seeks constraints upon his speculation in empirical data, anecdotal evidence, intuition, and parsimony. The degrees of freedom may be so reduced by these constraints that, within the confines of previous postulates, there is only one rational guess that will lead to theorems consistent with known facts. In such cases, rather than by trying out a series of random guesses, the theorist may be able to arrive inductively at the best postulate. The directness of the parallelism between an experimental and a theoretical function will then depend upon the nature of the function and the number of degrees of freedom available to the theorist.

The fact that Koch's analysis can reasonably be questioned is relatively minor; and it is certainly legitimate for Koch to have opinions at variance with Hull's. What is critical is that Koch uses his analysis to imply that he has somehow *proved* that Hull's general approach is wrong. It is important to recognize that such statements by Koch are ones of *belief*, for which history must provide the validation.

There is no doubt that Koch undertook an exceedingly difficult assignment, and the weaknesses in his analysis are certainly due in part to the necessity for him to limit his task. A more complete picture of Hull's theoretical approach would have required much more space, just as space limits the present reviewer in considering Koch's criticisms. Nevertheless, since Hull's postulate sets were not advanced as final ones, defense of points peculiar to them is, in this sense, trivial. Furthermore, readers capable of judging between Koch's charges and possible replies are already well acquainted with Hull's theory, and such persons can themselves separate Koch's valid from his defective criticisms.

A single paragraph must, therefore, suffice here for more detailed criticism. Let us state categorically those kinds of defects which, in this reviewer's opinion, recur in Koch's analysis of Hull. (a) Koch sometimes misleads his readers by describing the worst possible interpretation of Hull's writings. This may result from a too-literal reading of Hull, as, e.g., in Koch's assessment of the definition of response (p. 21); it may reflect an ungracious reading, as, e.g., in Koch's allegation that conditioned inhibition involves an association between one stimulus and another (p. 144); or it may involve a misinterpretation as, e.g., in those criticisms assuming that the quantification methodology involved a fundamental change in the technique of postulate construction and quantification (e.g., p. 136). (b) Some of Koch's criticisms are based on questionable methodological points, as, e.g., his assessment of what can properly be regarded as consequences of the theory (pp. 84f). (c) Koch blames Hull for things probably true of all theories in any field, as, e.g., the question of whether all of the determining variables are identified (p. 54). (d) Koch criticizes Hull for one behavior and also criticizes him for the opposite. For example, while, as noted earlier, Koch disapproves of Hull's "empirical" techniques of theory construction, he as often criticizes Hull for "rational" procedures (e.g., p. 118). (e) Koch overemphasizes the negative aspects of Hull's work which could as well be excused, as, e.g., the use of sH_R rather than sH_r (p. 60). (f) Koch makes statements with a finality unjustified by any evidence which he presents. For example, while he criticizes the paucity of data using the quantification methodology (e.g., p. 128), he concludes, without any further data, that the methodology is infeasible (p. 155). (g) Koch condemns actually good theoretical behavior, as, e.g., Hull's searching out the consequences of the new quantificational program (e.g., p. 121). (h) Finally, Koch makes some false statements, as, e.g., his assertion that p is not in the 1949 postulate set and the alleged reasons for its absence (p. 127).

Despite these defects, were he alive, Hull would certainly welcome such criticism, for his theory was vigorously programmatic and he hoped to inspire the advancement of competing systems as well as to further work on his own.¹ To

the extent that Koch has contributed to these goals, the results of his analysis will be salutary.

Koch's major contribution, however, may be found more in his identification of methodological issues than in his application of particular analyses to Hull's theory. This identification is important because few psychological theorists are competent to deal creatively with problems properly the subject matter of the philosophy of science. For example, Koch correctly notes that sL_R and sO_R "are at variance with Hull's metatheoretical conception of the nature of intervening variables, since they are not linked . . . to any 'observable' antecedent conditions at all" (p. 47). In this case, Hull's theory is sound but his metatheory has been overstated, since calculational devices, so long as they are not used differentially in making predictions, need be anchored only to consequences.

Let us summarize our comment. Koch presumes that his readers are already well acquainted with Hull's theory and makes a number of statements as to deficiencies in that theory and in Hull's general approach. Some of these statements are correct, some incorrect; but all must be tempered jointly by the fact that the Hullian program is still in an early stage of development, and by the fact that Koch disagrees with the fundamental methodology of that program. Koch's critical analysis must itself be read critically.



MacCorquodale and Meehl's Analysis of Tolman

By M. E. BITTERMAN

The Institute for Advanced Study

After a formal analysis of Tolman's system, which they find no reason to criticize "as a kind of behavioral theory," MacCorquodale and Meehl turn to a detailed consideration of experiments on latent learning and the adequacy of alternative interpretations. They emerge "somewhat more impressed" than they had expected to be by the "overall trend of the evidence" and prepared (though grudgingly) to admit



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that the outlook for expectancy theory is "at least encouraging." Thus struck with the possibilities of the system, but appalled by "its lack of even a minimal amount of formalization," the authors set themselves the task of recasting it in a more fashionable mold (at the calculated risk of blurring distinctive characteristics and "watering down" differences). The outcome is a Hullianized edition of Tolman, lacking the flair and scope of the original—not creative, only industrious; not bold, only determined; not provocative, only insistent; not comprehensive, only meticulous; not insightful, only clever—but better suited indeed to the contemporary psychological taste, and destined perhaps to play an important role in the ponderous transfer of mass allegiance, now unmistakably begun, from *S-R* reinforcement theory to a more reasonable view of mammalian motivation and learning.

The new formulation, which must be evaluated in its own right, represents a considerable advance over Hullian thinking.

First, it gives a central place to learning about the afferent consequences of response. Each instrumental action is traced to an expectancy ($S_1 R_1 S_2$) which is defined in terms of an expectandum (S_2) as well as an elicitor (S_1) and a response (R_1). In Hullian theory, the properties of S_2 can be anticipated only indirectly, by

way of proprioceptive feedback from characteristic responses to S_2 which become separately connected to S_1 . The elimination of mediating responses brings much greater flexibility with no loss of rigor.

Second, the new formulation incorporates a "stimulus-reinforcement view" which dispenses with need reduction as an essential condition for learning. The growth-rate of $S_1R_1S_2$ increases with the absolute valence of S_2 , and positive valence may stem in part from previous need-reduction (cathexis), but the possibility of learning with an unvalenced S_2 is admitted, and, furthermore, a negative S_2 (need increment) is held to produce as rapid learning as a positive S_2 of the same absolute valence. This change facilitates the derivation of latent learning and permits a more realistic interpretation of the effects of punishment than is afforded by Hullian theory.

Third, the new formulation recognizes the possibility of learning based on sensory conjunction alone, without the occurrence of response. Not only may a neutral stimulus acquire cathexis as a result of temporal contiguity with a valenced one, but under certain conditions new expectancies may be established as a consequence of purely afferent events. Given a *mnemonized* expectancy $S_1R_1S_2$, resulting from the close temporal sequence $S_1 \rightarrow R_1 \rightarrow S_2$, an *inferred* expectancy $S_1R_1S^*$ will be created by the temporal contiguity of S_2 with some valenced stimulus S^* . (The inference postulate, introduced to account for the results of certain experiments on latent learning, actually is unnecessary; the authors to the contrary notwithstanding, secondary cathexis will accomplish the purpose equally well if the mnemonized expectancy can be established with an S_2 of zero valence.) The door is thus opened to a more careful consideration of the question of perceptual learning.

THESE advances, it may be well to note, are made on a relatively narrow front—the new postulates, designed to "explicate not so much 'Tolman' as 'an expectancy theory,'" take no account of a variety of problems (such as perceptual organization, recognized by Tolman though treated somewhat cavalierly by him) which the authors consider "logi-

cally irrelevant" to the expectancy principle—and even on that narrow front a number of reassuring lines of communication with the popular Hullian tradition are maintained. The formal link alone should have tremendous therapeutic value; an expectancy labeled $S_1R_1S_2$ and introduced in the context of a quasi-quantitative postulate may perhaps engender no more anxiety than an sH_R . In addition, there are significant substantive continuities. For one thing, this is a *response* theory, admitting of no "environmental" expectancies. Tolman has been criticized for failing to pay sufficient attention to the translation of knowledge into action, and MacCorquodale and Meehl, in an attempt to avoid this difficulty, "put R in . . . at the beginning." Every bit of learned behavior is traced to the activation of an expectancy which is defined in terms of a specific response; even inferred expectancies, established by sensory contiguity alone, make explicit reference to response. Furthermore, tissue-need (albeit "nonphysiological") continues to play an important part in the new formulation. While valence takes the place of drive in the familiar multiplicative formula for reaction-potential, positive valence derives often from previous need-reduction (cathexis) and always from deprivation (need strength). Unfortunately, these ties to the Hullian tradition, which will undoubtedly win for the new formulation a certain amount of respectful attention that it otherwise might not receive, are maintained at substantial cost.

Consider the problem of form. In Tolman's hands, expectancy theory is loose and inductive, as befits the data. There are gaps, but the gaps are in full view. In the hands of MacCorquodale and Meehl, the emphasis is deductive, rigor is more apparent than real, and ignorance lies hidden in the postulates.

Consider the emphasis on response. With environmental expectancies ruled out, the theory cannot begin to deal with the phenomenon of classical conditioning (with which Tolman also had a certain amount of difficulty) or with the related findings which point to the development of what might be termed *elicitor-equivalence*. It should be obvious, for example, that conditioned salivation cannot be traced either to a mnemonized or to an inferred *SRS*. Meat powder cannot play

the part of S_2 to salivation's R_1 because the temporal sequence is wrong; salivation never antedates the *US* until the conditioning to be explained has taken place. Nor can meat powder be cast in the role of S^* (with the *CS* as S_2) except on the farfetched assumption of a previously established expectancy involving salivation as R_1 and the *CS* as expectandum. When a noxious *US*, such as unavoidable shock to a limb, is employed, there is a problem of valence as well as a problem of temporal sequence; with shock as expectandum, reaction potential becomes negative, and there can be no activation. Is it surprising that the authors should have failed to think through the relation of their formulation (however tentative or "programmatic" they may have considered it) to the Pavlovian paradigm? Not really; classical conditioning is quite out of vogue nowadays. But what of the more popular experiments on 'acquired drive'? A rat is trained to escape shock by R_1 (Stage I). Then buzz is paired repeatedly with brief shock under conditions in which the occurrence of R_1 is prevented (Stage II). In test trials, buzz alone is found to elicit R_1 at a level beyond that attributable to stimulus generalization (as determined in control runs). Comparable results are obtained when the temporal sequence of Stages I and II is reversed. The new postulates lead us to expect that buzz will be negatively cathected, but they do not help us to understand why buzz should elicit R_1 .

CONSIDER, finally, the importance attached to deprivation. While negative valence stems directly from the properties of stimuli, positive valence is said to depend on "the time interval since satiation." Some such lack of symmetry is undeniably necessary, but the conception fails to do justice to modern work on motivation. The evidence marshalled by Young clearly demonstrates that positive as well as negative effects may arise directly out of receptor-functions.

One may wonder, too, whether the questions on which the new theory is silent are in fact "logically irrelevant" to what its authors regard as their central concern. Can the problem of perceptual organization really be ignored with equanimity in the construction of a postulate intended to define the condi-

tions under which expectancies develop? Can the paradox of partial reinforcement be shelved pending the delineation of the basic learning categories? Here is the easy road to another premature crystallization (as Maier has put it) that may be as suffocating as the one which we have so long endured and from which we are only now beginning the recover.

In all, I prefer to take my Tolman untranslated and unabridged. The times being what they are, however, the edition at hand (like the journey of Mohammed to the mountain) will serve a useful function.



Verplanck's Analysis of Skinner

By MURRAY SIDMAN

Walter Reed Army Medical Center

MANY of the characteristics of the other monographs in *Modern Learning Theory* are shared by W.S. Verplanck's critique of B.F. Skinner. They are tightly organized, intricately reasoned, self-consciously rational, and logically sophisticated to a degree far exceeding the demands of the theories under consideration. It is tempting for a reviewer to play the game according to the same rules, simply adding a few more pages in kind. Verplanck's subject matter, however, posed one problem that did not have to be faced by the other critics. As Verplanck correctly pointed out, the theoretical constructions in Skinner's earlier writings have since been abandoned by their originator and by others working within a similar framework. Such concepts as "reflex reserve," "immediate reserve," "reflex strength," "envelop," are of historical interest only, although the data they attempted to explain are still with us. This review, therefore, will be confined largely to some of the less formal, currently relevant aspects of Verplanck's appraisal.

Along with his generally sound analysis of the above theoretical concepts, Verplanck has, in several instances, constructed intricate logical nets to catch empirical fish. As an example, we may cite the treatment of Skinner's definitions of stimulus, response, and reflex. Skinner's discussions of these concepts

may be called "theoretical" only in the sense that the problems treated are basic to any formal theory of behavior. Definitions resulting from the analyses were not, as Verplanck would have it, a set of theoretical constructs, although they have been utilized as such by some theorists. Skinner's method of conceptual analysis proceeds first by identifying the essential operations giving rise to the term. Once the basic operations have been identified, the concept is examined in its 'pure' form; that is to say, uncluttered by properties resulting from philosophical, logical, or other types of preconceptions.

Such operational analysis yields empirical definitions, e.g., of stimulus and response. For this reason it was somewhat surprising to read Verplanck's clearly implied, logically derived conclusion that experiments performed by Skinner and others on stimulus generalization, response differentiation, matching, even visual psychophysics with animal subjects, are impossible as a consequence of the definitions of stimulus and response. Perhaps Verplanck's logic foundered when he failed to distinguish between the definition of classes of events called stimuli or responses, and the specification, in physical terms, of particular members of these classes. While the label *stimulus* may be applicable to an event only after it has been shown to affect the behavior of the experimental organism, the pre-experimental specification of the event requires only that it bear some relation to the behavior of the experimenter.

Although Skinner's theoretical system has met the end that is inevitable for all such creations, his systematic experimentation continues to flourish. This is a state of affairs incomprehensible to many. Verplanck makes several attempts to wring some sort of a theory from the experimental data. A series of positively accelerated sections ("scallops") on a cumulative response curve becomes, for him, a theoretical issue, since a scallop may not be recognized by a person to whom cumulative curves are unfamiliar. The same criticism may be made of any visual or other sensory experience. Again, the operant-respondent distinction is treated as a theoretical whim. This dichotomy, however, can be abolished only by experimentation. It will be necessary to demonstrate that behavior known to be governed by its consequences can be conditioned according to a Pavlovian formula, and that Pavlovian-conditioned behavior may be manipulated by varying its consequences.

To be fair to Verplanck, it must be pointed out that he fully recognized that the type of analysis he was attempting would be considered irrelevant by proponents of Skinner's system. There are, nevertheless, some criticisms of the empirical aspects of the program that deserve clarification.

The outstanding characteristic of Skinner's approach is neither a rejection of theory nor a concentration on data gathering. It is, rather, a program of technique development. What are the most powerful methods for gaining control over the behavior of the individual? (Verplanck has made the common error of calling the Skinner box Skinner's "technique." It is difficult to understand why experiments on the many reinforcement schedules, on aversive techniques of control, on imitative and cooperative behavior, to name only a few, must be lumped together as one technique.) That technique development has proceeded more rapidly than has the assignment of numbers to the resulting data is a feature of Skinner's own investigative behavior, not a logical characteristic of his system. There have been enough instances of quantification to indicate that the techniques are highly adaptable to such procedures. But whether an experimental program is to be devoted to the discovery of new techniques or to the refinement of



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old ones will be determined by many factors, not the least of which is the current status of the science. It is not unreasonable to argue that Skinner's method of investigation yields the type of information currently most needed.

That the techniques of behavioral control double in function as techniques of analysis seems to be unrecognized by Verplanck. He apparently feels that the control of certain important variables is not sufficiently precise for analytic purposes. Yet, in the midst of criticizing the experimental control as ambiguous because temporal parameters of reinforcement are determined by the responding animal, he makes the astonishing statement that, "this ambiguity of experimental control serves to reveal new and orderly phenomena of behavior" (p. 290). In spite of Verplanck's labored demonstration that an orderly curve may, indeed, result from the chance alignment of a large number of uncontrolled variables, it would seem rather farfetched to attribute the continuing demonstrations of orderliness achieved by Skinner's experiments to a systematic and deliberate neglect of experimental control. The animals are not "free" to determine the temporal parameters of reinforcement, which are imposed by the schedules set up on the programming apparatus just as inevitably as if they were manipulated by confining the subjects in a retention chamber or by withdrawing the lever. Temporal and other variables are not only manipulated, but also identified, by means of scheduling techniques.

Thus we find experiments in which the traditional problem of delayed reinforcement is illuminated through a technique that takes advantage of the unique behavioral characteristics generated by a fixed-interval reinforcement schedule, experiments in which the temporal variables imposed by a scheduling tape are identified by programming different schedules in tandem, and experiments utilizing the technique of chained schedules under stimulus control to investigate the phenomena of secondary reinforcement. The latter studies have revealed powerful new variables that make many of the commonly accepted temporal parameters of secondary reinforcement appear insignificant. (Nor did Verplanck recognize data of this sort as one of the factors responsible for the "isolation" of Skinner's system.)

There is to be noted, finally, the cavalier treatment afforded conventional statistical techniques by Skinner and his associates, a treatment condemned by Verplanck, infuriating to many psychologists, and the despair of editors who must keep one eye on accepted practices and the other eye open for unconventional advances. Skinner's rejection of "confidence-level statistics" derives from his clearly stated interest in the behavior of the individual. This interest dictates an experimental design different from that generally used in psychology. Instead of running groups of animals and averaging their data, it becomes necessary to run individual animals through all of the experimental manipulations. Each animal thus constitutes a replication of the experiment, which not only affords an opportunity for detecting differences among animals, but also actually imposes the obligation to report them and, where possible, to *explain* them. The procedure of treating differences among animals as lawful, rather than as examples of the capriciousness of nature or of the experimental techniques, provides Skinner with one of his substitutes for statistical treatment. Experimentation is continued until the variables responsible for "deviant" behavior are identified. A corollary of this point of view is that any behavioral effect repeatedly demonstrated in the same animal is a lawful phenomenon. Failure to observe the effect in other animals reveals an ignorance of the relevant variables but in no way negates the original finding.

An additional substitute for confidence-level statistics is the repeated demonstration of behavioral phenomena in new experimental situations. For example, instead of using a large group of animals to demonstrate the behavior characteristic of fixed-interval reinforcement schedules, Skinner makes the original demonstration in two or three subjects. He then goes on to utilize the characteristic fixed-interval behavior as one of the tools for investigating conditioned reinforcement, chaining, delayed reinforcement, multiple stimulus control, drive interaction, drug effects, temporal discrimination, conditioned aversive stimuli, simple and complex discrimination. Thus each of these experiments, in addition to revealing new data, acts as a confirmation of the original finding. The process of systematic investigation con-

tinues, with the new findings generating additional experiments whose results serve to confirm or modify earlier conclusions.

Skinner's criterion of acceptability of experimental data is not the confidence level of the statistics, but one's degree of confidence in the competence and integrity of the experimenter. The experimenter's skill and integrity are maintained, in turn, not by his statistics but by the traditional principles of replicability by others and of the consistency of data when they are applied to new situations and with new techniques. A requirement of significance at the .001 level is in no sense an adequate substitute for the more laborious but eventually *self-corrective* process of systematic replication.

Strangely enough, Skinner's alternatives to statistical analysis are regarded as radical by many contemporary psychologists, including, apparently, Verplanck. In the sense that Skinner's mode of operation antedates the statistical approach and is regarded with the highest respect when used in other sciences, he may be considered one of the conservatives of modern psychology.



Estes' Analysis of Lewin

By DAN L. ADLER

San Francisco State College

FROM W. K. ESTES' assumption that "learning theorists" (defined as *S-R* or association theorists) are a Good Thing, it follows—for him—that "field theorists" are a Bad Thing.

Estes begins his polemic by noting the distinction between field and nonfield theories in physics and dismissing the notion that the distinction is meaningful in the realm of psychological theories. For him, no "true" field theory exists among the latter. This summary disposal of field theory in psychology is curious, indeed, until one realizes that Estes is claiming for field theorists what they do not claim for themselves—a complete (or even partial) analogue to modern physical theory.

A survey of Lewin's early articles (1917-1922), for example, would fail to

substantiate the accusation that "Lewin capitalized upon the immense prestige of relativity theory, proposing to revolutionize psychological theory by wholesale application of what he conceived to be the concepts and methods of physics" (p. 341). What Lewin did attempt—and this statement must be viewed in proper historical perspective—was to supplant scalar with vector notions of motivated behavior. If Lewin is "in bondage to mechanical models of classical physics" because he has espoused the concept of directed action (p. 320), he is at any rate accompanied by most 'learning theorists.' They suffer less merely because no one, as yet, has classified them as would-be physicists.

Estes' claim that field theorists have made little use of partial differential equations and presumably, therefore, do not qualify as field theorists, emphasizes only the question of how one may define field-theoretical psychology by criteria other than those of field-theoretical physics. Lewin has made this point clear, but nowhere does Estes acknowledge Lewin's comments upon the definition of the field at a given time (in this respect), although the appropriate reference appears in his bibliography. Neither does Estes appear to have read Lewin's discussion of constructs in field theory or his criticism of premature formalization in psychology.

After sounding the death knell for field theory, Estes dissects the remains—vector psychology and its learning aspects. Unfortunately, his program is not carried out with scientific regard for preservation, but with a carelessness appropriate to things which, if well buried, must certainly be dead.

Paradoxically, Estes complains that Lewin's coordinating definitions are not yet defined in terms of observable behavior or environmental events (p. 327), yet he condemns the topological and hodological structure because "in every one of these cases . . . the concept is coordinated to a psychological concept which refers to some aspect of behavior [italics the reviewer's], never to independently manipulable variables or independently observable events" (p. 335). What Estes undoubtedly seeks is a Skinner-type coordination; he fails to point out that Lewin coordinates definitions within the nomological network and tests the constructs (and their interrela-

tionships) by the application of independently manipulable variables and independently observable events. A glance at the literature on decision time, level of aspiration, rigidity, and satiation could readily establish this point. At the same time, these studies demonstrate that prediction is possible from Lewin's theory (see p. 332), that upon occasion it is derived from "experimentally isolated situations" (see p. 326), and that it is often directly applicable to the broader aspects of human behavior (see p. 326).

In so brief a review we cannot touch upon all of the issues raised by Estes. When they have been stated as issues, they have a rightful place in his critique. A



KURT LEWIN

certain amount of exasperation is engendered, however, by his introduction of imputation and pronunciamento, which have no place in an evaluation designed to be a methodological analysis. Unfortunately, these items constitute the foundation for his complete and irrevocable rejection of both the nomological network of Lewin's theory and its experimental fruits.

Perhaps, then, a note of caution is in place here. Just as constructs may be underdetermined or may vary in definiteness at different stages of research or may undergo changes commensurate with new observations, so too may the criteria of construct validity be altered. It is not for any psychologist to say that his are the true and ultimate criteria of such validity, nor that they are a reasonable basis for the annihilation of another's theory.



Mueller and Schoenfeld's Analysis of Guthrie

By EDWIN R. GUTHRIE

University of Washington

READING C. G. MUELLER and W. N. SCHOENFELD's analysis of my published statements on learning theory was, as I wrote Schoenfeld, much like viewing one's self in a mirror with better than adequate lighting. They had read everything relevant, had read with understanding, and had written with great objectivity. Their report brings out clearly the differences in our points of view. The chief difference concerns the nature of the data available to the psychologist describing learning.

We would probably agree that the occasions for action used by a stimulus-response account are, in any specific case, analyzable into component physical forces. They are light, sound, pressure, chemical action, and so forth; but I believe they are more than this, for we should never make a beginning toward the description and prediction of behavior in these terms. The important characteristic of stimuli as the occasions for action is their patterning. Patterns of physical stimuli are effective as *patterned* (not as patterns, as the Gestalt psychologists urged) and with very minor reference to intensity or other purely physical characteristics.

What is here meant may be approached from a different direction. From my point of view the questions that psychologists must undertake to answer (questions not faced by the physicist or the physiologist) are like: What will the kitten do the next time it sees the dog? How can this pupil's habit of responding 65 to 7×9 be changed to 63? Such questions involve observables different from the observables of physics and chemistry. Physical observations involve observer judgements in reading scales or in interpreting such patterns as the tracks of atomic particles on a sensitized plate. But the 'variables' with which psychologists must deal, though they are features of the real world and are physical events, are not describable in physical terms. They are classes of physical patterns of which the class characteristic can not be

stated in terms of centimeters-grams-seconds. The 'sight of the dog' must, like any observation in physics, be determined by an observer. No mere examination of its physical pattern of light can establish its function as a cue. The observer must not only observe that the dog is available for the kitten to see but also that the kitten sees it and is reacting to it.

A number of psychologists, including Loucks in our University of Washington laboratory, eliminate the complication of attention that confuses psychological observation by directly stimulating the brain electrically, and this promises to throw light on the nature of the learning process. In these experiments the physical stimuli that Mueller and Schoenfeld insist on can be used and well controlled; but the phenomena with which psychologists must deal in describing the behavior of intact animals are better named signals or cues than stimuli if we use the word *stimulus* in the sense which Mueller and

agreement can be reached in reading scales in physics; but even in physics the interpretation of the film track of an atomic particle is subject to some lack of complete agreement. Even so simple a thing as counting involves an observer's recognition of the recurrence of an event and an assumption that other observers would agree with him. Numbers and numbering are what the authors call *constructs*. Nature does no counting.

My notion of the basic problem in the field of learning is: How can we predict what the organism will do on its next perception of the cue? I have recently suggested a modified version of the general rule of associative learning which I believe offers a suggestion toward the answer. *What is being noticed becomes a signal for doing what is being done*. This rule has in intensified form all the faults which the authors (and, incidentally, I also) have pointed out in the previously offered rule of conditioning. The data language is ambiguous and vague. The only quantification that could be suggested would be to assign, after the manner of Estes, probabilities to the occurrence of the cue and to the occurrence of attention when the physical stimuli were present. I believe the curves of learning originate in the contingencies of the recurrence of stimuli and of attention. Voeks has suggested a postulate and Boguslavsky a statistical construct that would both make response probability a function of the number of repeated exposures to a situation, based on the enlistment of new cues. In my opinion the basic event in learning is that moment of change in which a new cue is enlisted for a response. This event is normally achieved on one association. If it were not, there would be no point in reading this page without being prepared to read it over and over again.

To my notion the nervous system of vertebrates is a machine for establishing new patterns of stimuli selected by attention as signals for the prevailing response; but the elaboration of this statement is obviously beyond the limits of this short review.



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Schoenfeld advocate—that is to say, in the sense of the local aspect of physical change. They point out that the extent to which we can observe that physical changes excite sense organs is open to question. Of course it is. But the signal character of a physical change is also open to observation. *Observers can agree that the cat sees the dog*. A higher degree of

A Summing Up

By MELVIN H. MARX

University of Missouri

THE REACTIONS of the present reviewers to *Modern Learning Theory* need to be considered in the light of two facts. First, reviewers were selected for their close association with the criticized theories. CP believed that the intensely critical tone of the original critiques justified—and in some cases demanded—an equally vigorous defense by an enthusiastic and active protagonist. Secondly, the reviewers were asked to emphasize major points of disagreement.

CP got what it sought—a set of strong attacks upon the original critics. Only Guthrie, observing himself in the 'mirror' provided by Mueller and Schoenfeld, is generous in praise of his critics, concentrating his discussion upon what he now sees as a single key difference of opinion.

The most important of the objections raised by the reviewers against the critics can be formulated as answers to three questions.

1. *Do the critics accept all "existing lines of attack"?*

In their joint introduction to the book the critics express a well-taken dissatisfaction with their slight success in clarifying essential similarities and differences among the various theories. They insist, however, that "our wish to do [this] . . . must not be taken as implying that we feel any of the existing lines of attack upon problems of learning should be scrapped" (p. xiii).

On the latter point three of the reviewers offer strong dissent. Adler flatly accuses Estes of a "summary disposal" of field theory. Bitterman believes that MacCorquodale and Meehl's "Hullianized edition of Tolman" has eliminated the essential virtues of the original. And Logan finds in Koch a fundamental rejection of the hypothetico-deductive method, pointing to this as a key factor in his treatment of Hull.

The dissent of Adler and Bitterman might have been expected on the basis of their 'field-theoretical' background, since the original critics are obviously of 'S-R'

persuasion. Guthrie and Sidman, as *S-R* reviewers of *S-R* critics of *S-R* theories, do not make this kind of complaint.

2. Do the critics adequately examine the relationship between formal theory and research stimulation?

Important questions are raised, if not answered, in relation to this general problem, which is set forth as a major concern of the Dartmouth Conference out of which the book originated. Logan sees a crucial paradox posed by Koch's devastating critical analysis of the formal inadequacies of Hull's quantitative system and his recognition of its unexcelled contributions in the stimulation of research. Is it some peculiar aspect of formalization per se, or merely the *specification* and *explication* of a number of concrete postulated relationships, that accounts for the great success of such formal systems as Hull's in stimulating research? Sidman criticizes Verplanck for undervaluing, in the absence of formal theory, the systematic research contributions of Skinner's experimental techniques. Is formalization in advance necessary for the ultimate development of good research-supported theory? Bitterman objects to the particular kind of formalization offered by MacCorquodale and Meehl (although it is not clear exactly how any formalization can proceed without the loss of at least some of the flavor and dash of the original notions). Where is the "ignorance" best placed—in the "gaps" in the theory, as Bitterman sees it in Tolman's expectancy theory, or "hidden among the postulates," as he finds it in the *S-R* formalization?

3. Do the critics hold too rigidly to high critical standards that are not always appropriate?

This is obviously a complex question that cannot be given a simple and unqualified answer. On the positive side, a major contribution of the critiques is their particularized demonstration of the many pitfalls that await the unwary in the difficult and largely uncharted areas of theory construction. This emphasis, if applied in proper perspective from the very beginning of theoretical enterprise, would certainly seem to be a salutary influence.

On the other hand, a number of serious questions can be raised as to risks in-

curred by what seems in some cases to be an inflexibly high level of theoretical aspiration and an all-or-none criterion of theoretical success. It is true that the critics deliberately set for themselves the task of being as rigorously critical as possible. It may also be true that we can learn as much from failures as from successes, and that "the strictest criticism of current formulations is necessary to facilitate further development" (p. xv). This rule, however, applies most directly to theories in a relatively advanced stage of development.

UNFORTUNATELY, as Sidman observes, the logical sophistication of the critiques is far in excess of anything demanded by the criticized theories. Production of theories *worthy* of this high-level criticism is an immediate task confronting present-day psychology. Overconcern with problems of form and current orthodoxy in the initial phases of the development of theory may well tend to stifle creativity and channel thinking into narrow and circumscribed grooves. This fear constitutes the gist of the complaints tied by Adler and Bitterman.

The failure adequately to emphasize intermediate stages between present achievement and ultimate objective can also serve to discourage the approximate or tentative theory that seems to be needed in early phases of development. Moreover, measuring a formal theory in its entirety against the loftiest of critical

standards may cause one to overlook the contributions actually made in the various more circumscribed parts of the theory. The location of promising "miniature systems" was an explicitly stated objective of the Conference, one which does not seem to have been realized in the book.

Finally, the relationship of *Modern Learning Theory* to the era of psychological theory construction which it examines may itself be examined. The book plainly shows many of the characteristics of the era. Its publication can certainly be credited with at least a strong assist in the present tendency toward disenchantment with formal theory. A welcome consequence of this shift in climate would be the elimination of certain of the less desirable characteristics of the recent period—especially, perhaps, the personal glorification and vilification resulting from an overemphasis on systematic allegiances. In the theoretical vacuum which now threatens we need a calm but unrelenting pressure, on a wide variety of subject-matter fronts and with a wide diversity of lines of attack, pressure toward the accumulation of sound theoretical explanations of behavioral functions.

Modern Learning Theory is a rigorous logical exercise which has made few positive contributions towards such a development. Nonetheless, it is a fitting capstone for the recent period and offers plenty of criteria against which formal theorizations can some day be measured.



A man's writing is himself. A kind man writes kindly. A mean man writes meanly. A sick man writes sickly. And a wise man writes wisely. There is no reason to suppose that this rule does not apply to critics as well as other writers.

—JOHN STEINBECK



Did Freud Invent Human Nature?

W. David Sievers

Freud on Broadway: A History of Psychoanalysis and the American Drama

New York: Hermitage House, 1955.
Pp. 479. \$5.00.

By HAROLD G. McCURDY

University of North Carolina

THE AUTHOR of this very solid work, the public result of a 1951 PhD dissertation at the University of Southern California, is to be thanked for providing us with convenient summaries of hundreds on hundreds of plays and thereby passing in review the offerings of the American stage, major and minor, during the past half-century. It would be well, however, if he could be pleasantly sentenced to exile in some bookless land to treat his abundant raw materials to a distillation process and to reflect on his title. He has left the reader too much to do.

Sievers tells us that it was his first intention to call his book "From *Suppressed Desires* to *A Streetcar of the Same Name*." It is a pity that he did not. The frivolous would not have imagined that some strange episode in Freud's career was about to be uncovered; the serious would not have expected an elucidation of a difficult aspect of cultural history. The book falls short of the latter aim through the very plethora of its facts. On the other hand, in strict accord with the rejected title, it does indeed chronicle the masks and transmogrifications of the libido as these have frequently, variously, and persistently been canvassed by American playwrights from about the time of Susan Glaspell's "delightful satire on the effects of amateur psychoanalysis in the hands of a giddy faddist" up to and slightly beyond Tennessee Williams' clangy trolley.

As to the style of the book, I suppose that it could not have been made more light and airy without defying the entire PhD system. The cabinet of filing cards rules our learning. Dr. Sievers writes good sentences regularly, good paragraphs often; but his theoretical framework and aesthetic cunning are not quite adequate to keep the structure as a whole

from threatening to collapse into its elements, to disintegrate into the aforesaid filing cards, so conscientiously and ably gathered and so necessary for survival in this age of the graduate school.

THE point about which the carping reviewer feels most quarrelsome, however, is the author's wanton attribution of any and everything to that Great Man whom the title establishes so much more firmly on Broadway than the book does. An early chapter demonstrates that Sievers understands Freud's theory and terminology well enough. But a partisan spirit carries him headlong through the most obstinate counter evidence when he is in the mood to claim another scrap of territory for the Master. For example, he utilizes a questionnaire and other sources for remarks by the dramatists themselves as to the relation between their work and psychoanalysis; yet whether their testimony is yea or nay matters not at all; in either case the witness is liable to find himself in the procession of captives trailing after Freud's triumphal car. Eugene O'Neill is the epitome of the Freudian era for Sievers; his masterpieces are eminently Freudian, and he is not to be allowed to sneak out of it by saying: "There is no conscious use of psychoanalytical material in any of my plays. All of them could easily have been written by a dramatist who had never heard of the Freudian theory and was simply guided by an intuitive psychological insight into human beings and their life impulsions that is as old as Greek drama." Maxwell Anderson likewise puts up a stout defense, also calling the Greeks to his aid. And so do others. It does them no good. Protesting victims are herded into the same category as those who declare that Freud was their mother's milk and the breath of their artistic life. To be sure, there is the Unconscious, and all its tricky ways. But, then, what was the use of the questionnaire?

ONE is plunged into a brown study by this cavalier attitude toward evidence. Must the adoration of every great leader, intellectual or political, require that history be rewritten so that his name can appear on all the monuments? In what sense is a playwright Freudian, if he denies the influence? Can Seneca escape

the label, or Euripides, just because his work was done ages ago? Did Aphrodite anticipate Freud, or did he generate her from the seafoam? As far as Sievers is concerned the answer is simple: Anything is Freudian which either makes use of the Freudian lingo or presents human situations like those described by Freud. Now this is very loose definition. The lingo can decorate a play from which every trace of psychoanalytic insight is absent; the human situation may be observed as Freud observed it, quite independently and without his meanings. To speak of either of these cases as Freudian is to mislead and obfuscate, to befuddle and be befuddled. By this kind of language sex becomes Freudian, murder becomes Freudian, dramatic tension becomes Freudian, anything excitingly morbid or piquantly distressing becomes Freudian, and the only thing not Freudian is what one does not happen to like as a dramatic critic. So T. S. Eliot is taken to task for handling psychoanalysis with poetic license in *The Cocktail Party*, the crime being that his therapist "bears no resemblance to anything known and sanctioned by psychoanalytic societies." Sievers is moved to such compassion for the characters in the play who have fallen into the clutches of Eliot's phony that he wants to urge them to go and seek "legitimate psychiatric help" elsewhere.

Let it be understood that the foregoing criticism applies to the trimmings and tone of the book rather than to its main body. For the hundreds of plot summaries packed into the 440 pages of text we must be grateful; they are carefully and intelligently written and should be useful to a variety of specialties. In the interstices of this encyclopedic mass, however, there is enough commentary indicating that Sievers regards the future of the drama and mankind as lying with psychoanalysis to justify some attention to the definition of this hope in his system of thought. In my opinion, a less partisan attitude, a sharper definition of terms, a greater historical perspective, would have resulted in a better book. At the same time it must be acknowledged that Sievers was attempting a labor of Hercules when he undertook to show the influence of Freud on the drama—a tremendous labor, because it involves a question which Hercules Freud himself posed and never answered.—Who wrote *Oedipus Rex*? Freud or Sophocles?

CP SPEAKS . . .

A SCIENCE is its books. Or isn't it? Some academic investigators feel that a colleague who interrupts his research to write a book is definitely on the downgrade, deteriorating into the verbosities of aging, not rising at long last to the greater perspectives of maturity. Yet observation and experiment and statistical analysis are not enough for science unless they presently issue in publication, and the many articles that implement this step in the scientific process need, after they are printed, discussion, correlation, interpretation, and assimilation into a perspective. All such later organization and reorganization of what research has provided is the books, the books which contain the science of the decade in which they were published.

So the science of a particular time, if not exactly its books, at least is contained within the books of that period. What, for instance, was psychology in 1856, when the new experimental movement was getting ready to be born? Lotze, Spencer, Bain, Helmholtz and some others who formulated what was psychology before the decade of Fechner and Wundt. If you want to experience the psychology of a century ago, read these books. Just so *Contemporary Psychology: A Journal of Reviews* now undertakes to exhibit the contemporary scene within the no longer new psychology of 1956. If psychology is in its books, then "CP"—as those providing prenatal care for the new journal already affectionately call it—can picture the current scene by telling its readers what the recent books contain.

CP, however, does more. Its reviewers are being asked not merely to abstract books but to criticize them both negatively and positively, to put them in perspective, and to suggest their significance in modern psychology. Often this sort of expansive positive criticism, when well done, makes separate abstracting unnecessary.

The instructions that CP sent to the reviewers contained the question: "Can

not psychologists write well?" The embryo CP wondered, hoped and was not too sure. CP means to be interesting, and interesting writing is good writing. Now this first number of CP begins to answer the question and CP is pleased. Psychologists can write better than CP feared at first. There are, indeed, individual differences among psychologists, in respect of wit and wisdom, and in respect of being clear and being pleasing. Yet CP hopes not to be dull. We shall see presently what incentive and selection can achieve.

TO WHOM is CP to be interesting? First to American Psychologists, the APA's fourteen thousand and the others. CP is not the place for electroencephalographers to write to electroencephalographers. It is the place for electroencephalographers to write to religious psychologists who wish they were something more than religious psychologists, and for religious psychologists by being irresistibly interesting to usurp the attention of electroencephalographers. But that is not all. Psychology has its sociotropic and biotropic peripheries who may listen when it speaks. Psychiatrists, anthropologists, sociologists, philosophers, historians even—biologists, physicists, statisticians, and some mathematicians—none of these is immune to interest in psychology. And beyond them are the persons working in government, industry, and business, and all around the outside are the intellectuals, the people who like to know what is going on in new thinking. *Contemporary Psychology* has something to say to all these minds, and CP has its eye on the people who own the minds.

If CP is a journal of criticism, how do you keep the criticism fair? You don't. You just put up with having human beings for reviewers. At times, however, CP will have double or triple reviews of important books, especially when contrasting reviews can be predicted. (CP has a mathematiphile-vs.-mathemati-

phobe review in the works now.) CP will also have a department of letters, appearing under the title, ON THE OTHER HAND . . . For more on this problem of letters, see the introduction to that section (p. 29). There have to be limitations, of course, but, to the ancient convention that authors must always suffer a depreciation in silence, CP says "Phooey!"

CP operates through Consultants, twenty-six of them as listed on the inside cover of this number. They have their special fields, but the fields overlap so intricately that CP does not specify them in print. The Consultants advise the Editor as to whether a book should be reviewed or not, how long the review should be, and who should be asked—nos. 1, 2, 3, 4—to write the review. They do not invite the reviewer nor edit his comment.

FILMS is a department especially emphasized in the ordinance that created CP. Dr. Adolph Manoil is CP's special Film Editor. For what he can do, see his department in this number and the next; for CP had to split his first offering in two in order to balance the boat.

THIS month the feature is a multiple review of multiple-authored *Modern Learning Theory*, a composite achieved by the labors of Melvin Marx who sums up the debate at the end. The editorial office has not lacked its wits who remark how appropriate it is for CP to be started off by Marx. That ephemeral pun will, however, soon fade away, while CP and Marx—psychology's CP and psychology's Marx—still endure.

CP SPEAKS . . . will not always be an exercise in homiletics. CP wishes in this space to discuss policy a little and books a lot. It asks its readers to tell it publishable gossip about forthcoming books, the sure ones and the probable ones, and it is extending the same invitation to publishers. This, Reader, is your invitation! What is going on bookwise where you work? (Share your value judgments and CP will not tell on you.) Is it going to be important? Why? Do you think the author will really finish it? When, would you suppose? Did he say it would be all right with him if CP decides to print some of this information?

One word more. CP wants to serve its readers—or at least their central tend-

(continued on page 22)

Savage Statistics

Leonard J. Savage

The Foundations of Statistics

New York: John Wiley & Sons, 1954. Pp. xvi + 294. \$6.00.

By WARD EDWARDS

Armament Systems Personnel Research Laboratory, Air Force Personnel and Training Research Center, Lowry Air Force Base, Denver, Colorado

A VERY distinguished mathematical statistician at the University of Chicago, L. J. Savage, has written a book which has three main properties. It presents a new theory about the logical and mathematical foundations of statistics and probability, a theory which centers on the notion that the function of statistics is to enable people to make wise decisions in the face of uncertainty. It also presents an extensive account of that branch of modern statistics called decision theory, and an interpretation of it in terms of Savage's views about the foundations of statistics. Most important to those of us who find mathematical statistics hard going, Savage's book is beyond comparison the most clearly, interestingly, and amusingly written book in the area of decision theory, and one of the two or three best written books in all of mathematical statistics—at least to this nonmathematician. For an adequate exposition at a nonmathematical level of such concepts as utility, subjective probability, decision functions, the minimax principle, criteria for point estimation, there is now no other satisfactory source; this book makes all previous nonmathematical expositions obsolete. And yet the complete mathematical content is there too. Unless you are a mathematical statistician yourself, you can expect to have your mathematical skills strained to and beyond their limits while reading this book. But even if you have trouble doing algebra, you will get a clearer idea of the basic concepts of modern decision theory than is available from any other source.

Savage's main purpose is to present his view about the foundations of statistics. He starts from the assumption that the purpose of statistics and probability theory is to give principles about how

acts should be chosen in the face of an uncertain future. He intends to accept only those principles about such action which, in his view, would necessarily be accepted by any reasonable man. He finds only two such principles. The first is that a person should always be able to put a set of acts into a simple ordering; that is, of any two acts, A and B , A is either better than, worse than, or just as good as B , and these relations are transitive. The second is what Savage calls the surething principle. Mathematically it is a bit complicated, but the essence of it is that if act A is at least as good as act B for all possible states of the world, and definitely better than B in at least one possible state of the world, then A should be preferred to B .

From these two postulates, plus a number of others which have only technical importance, he proceeds to develop first what he calls a qualitative personal probability measure, which essentially provides a simple ordering in subjective probability of all events. Then by means of one more technical postulate he transforms that qualitative personal probability into a quantitative personal probability. That final technical postulate is not as innocuous as the others; essentially it says that if event X is less probable than event Y , then a set of possible events, one of which must occur, can be found which is so fine-grained that the probability that either any given member of the set or X (or both) will occur is still less than the probability that Y will occur.

Quantitative personal probabilities, or subjective probabilities, are not the same sort of animal as the probabilities you were taught about in Statistics 1. They obey the same mathematical laws (e.g., the addition theorem), and they represent a kind of estimate of the likelihood of an event, but there the resemblance ends. A personal probability is a subjective estimate of a likelihood; to be more precise, it is a number that represents the extent to which an individual thinks a given event is likely. It is not necessary, therefore, that the personal probability of A be the same for different individuals. This does not mean, however, that individuals may freely choose personal probabilities and still behave in accordance with Savage's postulates. They can indeed do so prior to the first occurrence of an event; thereafter, the change in subjective

probability as a result of experience is governed by Bayes's Theorem. This means that if two people observe a series of coin flips, they may start out with personal probabilities of throwing heads which differ widely from each other, but after a number of flips they will end up with personal probabilities very close to each other and to the ratio of heads to total flips.

The notion of personal probability is not in itself enough to provide the basis for decision making in risky situations, but the rest of the theory is more or less conventional. It involves derivation of the notion of subjective value or utility. Then the best course of action is that with the highest expected utility, where expected utilities are calculated using personal probabilities. That completes Savage's view about foundations of statistics.

SAVAGE discriminates three kinds of views about what a probability is: objectivistic (the usual one), personalistic (Savage *et al.*), and necessary (Keynes *et al.*). Consider these two sentences. (1) The probability is high that the president of the U. S. in 1984 will be either a Republican or a Democrat (i.e., not a member of a new party). (2) A *t* test shows that, if the null hypothesis is correct, then the probability that another sample from the same population would have a difference in mean values as large as that obtained in this sample is less than .05. The essential difference between objectivistic and personalistic views is that objectivists assert that the word *probability* means something quite different in these two sentences; more precisely, they deny the formal meaningfulness of sentences like the first. Savage, on the other hand, would say that the two sentences use the word in the same sense (in fact, later chapters suggest that he would be more likely to use sentences like the first than like the second). It is apparent that many of the old and knotty problems about probability theory readily yield to this way of looking at probabilities. The principle of insufficient reason, for example, can be seen as a psychological law about the initial choice of probabilities, rather than a dubious law of mathematics. The uncomfortable status of Bayes's Theorem vanishes, and

the theorem itself emerges as the central law about change of probability with experience—as indeed it should, since it is just about the only law on that subject in all of probability theory. Professor Boring has expressed doubts that the frequency definition of probability can be of any use in connection with finite sets of observations, since the frequency definition concerns ratios of infinite classes and so is consistent with any possible finite set of events. These doubts, of course, vanish with the abolition of the frequency definition of probability.

Since this is not Utopia, new difficulties arise to take the place of the old. I believe Savage satisfactorily demonstrates that personal probabilities can be used for most ordinary statistical purposes. But, as he himself points out, we rarely know what our personal probabilities are, and indeed they are usually quite vague. Furthermore, the fact that personal probabilities may differ from person to person raises difficult problems when decisions affecting more than one person must be made. The remainder of the book is devoted to expedients for coping with these problems. These expedients center around the minimax principle. After an astonishingly lucid exposition of the logical as well as of the mathematical content of the minimax principle from the objectivistic point of view, Savage proceeds to reinterpret it as a rule for group decision. The essence of the rule, in this view, is that the decision chosen should be the one that results in the smallest maximum harm to any group member. In this spirit Savage goes on to present a stimulating discussion of more-or-less standard topics in mathematical statistics: observation, partition problems, point and interval estimation, and testing. This discussion seems far less well integrated with the radically unconventional views in the earlier part of the book than Savage might have wished.

Psychologists may be startled to find that Savage makes persuasive cases against testing hypotheses which assert that a function has a particular form, against the general use of dichotomous hypotheses, against the view that point estimates should be accompanied by estimates of the error of estimate, against confidence limits, and against several other similar psychological customs.

The most serious lack that I felt while reading the book is that Savage almost completely ignores current experimental work on utility and subjective probability. I have, however, an ax to grind in making any such criticism, so it should not be taken too seriously. A more serious criticism concerns Savage's view, shared by many other decision theorists, about the empirical content of decision theory. All decision theorists agree that the main content of statistical decision theory is normative; it tells you what to do. But decision theorists often ascribe an odd kind of empirical content to their theories. For instance, Savage says: If it is brought to my attention that I have violated the principles presented in this book, I feel that I have made a mistake, and, if possible, I rectify it. Why does Savage expect his readers to be interested in this statement and in about three pages of examples with which he illustrates it? Presumably it is because he thinks it possible that others are like him, and so that it is a general psychological law that the principles of decision theory compel conformity when understood. That general law is demonstrably false. The status of decision theory would be clearer if its exponents would maintain a sharper distinction between normative and empirical theoretical statements.

One of the effects of the view that statistical problems are decision problems is a severe reduction in the importance of measures of variability and of tests of significance. If decision *A* is better than decision *B*, the fact that the data proving the superiority of *A* to *B* are highly variable is not relevant to the choice between them, except as it contributes to the vigor with which *A* is recommended. Thus tests of significance influence only that vigor. This state of affairs is disturbing to old-fashioned amateur statisticians like me. Information about variability ought to be more important than that. Are there unsolved mathematical problems here?

And now a value judgment! If I had to select for my library only one of all the current books and articles in this area, I would not hesitate a moment before picking this one. And if, around 1960, someone were to give me enough money to buy a second book, my first question would be, "Has Savage published another book yet?"

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Concordance in Twins

Eliot Slater with the assistance
of James Shields

**Psychotic and Neurotic Illnesses
in Twins.** (Medical Research
Council, Special Report Series, No.
278)

London: Her Majesty's Stationery
Office, 1953. Pp. 385. £1 1s. New
York: British Information Services.
\$4.75.

By NICHOLAS PASTORE

Queens College

THE MENTAL hospitals in the London area, in the period 1936-1947, cooperated with the authors in uncovering the 297 twin pairs that form the basis of these intensive statistical and clinical analyses. The degree of concordance in the twin pairs as a function of ovularity and diagnostic classification is given in the table. The significantly greater degree of concordance in uniovular twins is strong evidence for the role of predisposing biological factors in mental illness. In an important feature of the study, the authors demonstrate significant differences in onset, course, and outcome of what is ostensibly the same illness in concordant twins. In addition, the authors assess the role of a host of psychological and environmental factors in relation to the degree of severity of illness.

The authors found only a few cases in which the diagnostic category of the propositus was not matched by a similar diagnosis in some pathologically affected

relative. (A *propositus* is the person or case that provides the starting point for a particular investigation.) This finding they interpreted as sustaining the view that "it does seem that schizophrenia and affective psychoses are genetically distinct" (p. 69). The validity of such a view rests on further investigations. The psychiatrist, however, has the opportunity, because of the many and detailed clinical descriptions which are included in the monograph, to judge the correctness of the authors' contention.

It is interesting to note (Table 10, p. 34f.) that there is a disproportionately larger degree of concordance (for schizophrenia) in female twin pairs (whether uniovular or binovular) than in male twin pairs. In alluding to this relationship (without explicitly stating it as the reviewer does), the authors write: "We can only note this fact as a curiosity and cannot offer an explanation" (p. 56). Might this relationship mean that there is a greater degree of psychological 'contagion' between one twin and her sister than between one male and his twin brother or sister?

AN ATTEMPT to formulate the specific genetical basis of mental illness involves information on the incidence of a given illness in parents, sibs, and offspring of propositi. Such an attempt is beset with many difficulties—procuring reliable information on relatives not available for observation, differing risks for admission to a mental hospital, etc. In any case, the reviewer disagrees with the authors' statement that among the parents of the schizophrenic propositi there were "7 cer-

tain and 5 probable schizophrenics" (p. 56). By actual count the reviewer found that only four parents were specifically declared by the authors to be schizophrenic and three parents as probable schizophrenics. Perhaps differing definitions and incomplete presentation of relevant data are responsible for the discrepancy. For instance, on page 115 the reader is promised complete case histories of all uniovular twins; yet in the following pages the case histories of only 24 of the 28 concordant uniovular twin pairs are given. The authors, however, give the impression of having enumerated all cases of affected relatives.

The number of affected offspring of the propositi and their co-twins was not excessive. Thus, from the case histories the reviewer found only two children who were abnormal of a total of 32 who had reached the age of 40 years.



Freeman in Revision

Frank S. Freeman

Theory and Practice of Psychological Testing. (Rev. Ed.)

New York: Henry Holt & Company, 1955. Pp. xvi + 609. \$5.25.

By H. GLENN LUDLOW

University of Michigan

PSYCHOLOGISTS and related professional personnel who have been pleased with the first edition of this book will find the revised edition an even more useful and complete volume. Critics of the 1950 publication will note several important modifications which should result in greater popularity of the present text.

The new edition reflects approximately six major changes. First, Chapters 2 and 16 of the old edition have been deleted. These chapters were, respectively, "Statistics in Psychological Testing" and "Applications and Problems." Second, the author has extended the scope of his discussion of test standardization with particular emphasis on methods of ascertaining validity and reliability. Third, new tests and relevant research materials have been included. Freeman appears to have turned in an excellent performance in

NUMBER OF CONCORDANT AND DISCORDANT TWIN PAIRS AS A FUNCTION OF OVULARITY
AND CLINICAL DIAGNOSIS OF PROPOSITUS

Status of Propositus	Number of Twin Pairs		Number of Co-twins Similarly Affected	
	Uniovular	Binovular	Uniovular	Binovular
Schizophrenic	41	115	28	13
Affectively ill	8	30	4	7
Epileptic or organic	9	37	4	2
Psychopathic or neurotic	8	43	2	5
Total	66	225	38	27

(Reviewer's adaptation of Table 10. Six pairs of doubtful ovularity have been excluded.)

his selectivity in this respect. Fourth, the proponents of aptitude testing will be happy to find approximately twice as much space devoted to this classification of tests. Two chapters cover the measurement of aptitudes for mechanical and clerical activities, the fine arts, and the professions. Fifth, the treatment of projective techniques has been extended so as to be more useful to students not specializing in clinical psychology. These chapters appear to be especially well written for consumers of the results of projective analyses. School psychologists, counselors, and guidance workers will be able to obtain valid, fundamental information from the author's straightforward evaluation of the Rorschach, the TAT, the Michigan Picture Test, and related tests. Sixth, the new edition gives more attention to the psychological analysis of the functions being tested by each of the several types of measuring devices. In this connection, Chapter 3, "Definitions and Analyses of Intelligence," is as good as the reviewer has ever read.

The foregoing changes are contained in a larger book of 609 pages (21 chapters) as opposed to the former publication of 518 pages (16 chapters). Approximately one-half (11 chapters) of the content is concerned with the measurement of intelligence. The remainder is divided by chapters as follows: one chapter on basic theoretical principles, one on interpretation of test scores, two on aptitude testing, one on achievement, two on personality rating scales and inventories, two on projective methods, and one on situational tests.

THIS text is no mere catalog of available psychological tests and instruments. The author's avowed objective of giving readers an understanding of the theoretical principles and assumptions upon which tests are constructed is magnificently attained. Although Freeman is objective, fair, and scientific in his writing, he does not hesitate to take a stand. For example, in justifying the limitation of his discussion of occupational inventories to the Kuder and Strong, he says:

Other inventories of occupational interests have been published; but few, if any, of them have been subjected to the extended

and intensive research done on the Kuder and Strong inventories. Practically all of the others should be regarded as tentative and in their early experimental stages.

Tests and principles are evaluated and criticized in a lucid way. The question and discussion technique employed in the sections dealing with the Stanford-Binet and Wechsler tests are fine examples of good professional writing.

One of the best contributions is the account of the historical background of the Binet scales. The reader is taken through the process of defining intelligence, and devising, refining, and standardizing intelligence tests. Although some might consider that too much space and attention are given to the measurement of intelligence, the reviewer does not consider this emphasis to be a weakness. Actually Freeman uses this area of testing as a vehicle for illustrating the major psychometric struggles through which pioneers must pass, regardless of the particular behavior they are endeavoring to assess.

Theory and Practice of Psychological Testing would have been further improved by a little more attention to the criterion of teachability. Surely problems and exercises inserted at appropriate places would assist student readers materially in the reflective process as well as ease the burden of instructors. Also brief, well-selected, and annotated chapter reading lists would be a welcome addition. Another question of less importance occurs in connection with the loss of the lists of tables and figures available in the first edition.

Students of education will be disappointed at the scant treatment given to achievement testing. It may well be, however, that, like current textbooks of United States history, the field of testing is getting too large to be covered by one volume. Some readers will consider the omission of the chapter on statistics a distinct loss. In general one may ask: How long will psychologists continue to ignore the results of their own experimental evidence on retention?

All in all, this publication represents a real addition to the field of psychological testing. It is an attractive, thoroughly readable, sound treatment of the subject. Dr. Freeman is to be complimented for actually revising a "revised text."

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Work Groups

Stanley E. Seashore

Group Cohesiveness in the Industrial Work Group

Ann Arbor: University of Michigan Press, 1954. Pp. vi + 108.

By GILBERT K. KRULEE

Tufts University

To THOSE psychologists who prefer empirical studies that are related to theoretical considerations, this monograph should be of considerable interest. It is another report in a series conducted by the Human Relations Program of the Survey Research Center of the Institute for Social Research. This study is concerned with primary group phenomena and with the consequences of group membership in groups of varying degrees of cohesiveness.

Seashore begins with a statement of his theoretical orientation and with certain hypotheses that can be derived from this theorizing. The main concepts used in the investigation are derived from work of the Research Center for Group Dynamics, all of which clusters around the concept of group cohesiveness. One set of hypotheses stems from the view that "membership in a cohesive group may be positively related to mental health, adjustment, feelings of security and, in consequence, to the reduction of some forms of anxiety." A second set of hypotheses takes up relationships between degree of cohesiveness and productivity standards. Seashore assumes first of all that the more highly cohesive groups exert the greater pressures toward uniformity of conduct among their members. He then develops auxiliary hypotheses so as to predict whether or not this uniformity will be in the direction of high or low productivity. The final section of this theorizing is concerned with certain determinants of the degree of cohesiveness.

Having made a series of empirical derivations within each of these theoretical areas, Seashore proceeds to test the derivations against survey data which were collected in a large Midwest factory. From these data he derives an index of

group cohesiveness which he then correlates against responses to the various questions which were also included as part of the survey. On the whole, the hypotheses within the first two areas of interest were confirmed, although the results concerning the determinants of group cohesiveness proved to be inconclusive and ambiguous.

The results indicate that members of the highly cohesive groups exhibit less anxiety than do members of less cohesive groups. Seashore used the following measures of anxiety: (a) feeling "jumpy or nervous," (b) feeling under pressure to achieve higher productivity, and (c) feeling of lack of support from the company. Similarly, the highly cohesive groups appear to exert stronger pressures on members to conform to group standards for either high or low productivity, with the direction of the influence depending upon the degree to which the group members perceive the company as providing a supportive setting for the group.

On the whole, this is a well-executed study and it is encouraging to see that theorizing based upon so-called 'laboratory experiments' does indeed appear to have usefulness for the interpretation of social phenomena in an industrial setting.



Thoughts on Counseling

Vivian H. Hewer (Ed.)

New Perspectives in Counseling

Minneapolis: University of Minnesota Press, 1955. (Number 7, Minnesota Studies in Student Personnel Work.) Pp. 60. \$1.50.

By CHARLES F. WARNATH

Teachers College, Columbia University

HERE is a collection of four papers from the Fourth Annual Conference of Administrators of College and University Counseling Programs. None of the four is closely related to any of the others except that each focuses on something about counseling. Each of the papers is readable, but the treatment of topics is uneven. There is a

paper by Cronbach relating communication theory to counseling. It is followed by a discussion by Williamson identifying seven forces in the development of counseling—the first five of which are given a more or less cursory treatment with attention centered on the budget problems of the counseling center and a defense of the integration of counselors and discipline. The third paper, by Black, discusses the use of the MMPI with normal persons—specifically college women. The final paper by Clendenen deals with the problems of the selection and training of counselors; most of this material is, however, a description of the practices of the UCLA Counseling Center.

One might justifiably ask on what basis articles dealing with theory, administration, a specific test, and a specific counselor selection and training program are lumped together. A more descriptive title and a short preface would have been helpful to the potential purchaser.



Behavioremes

Kenneth L. Pike

Language in Relation to a Unified Theory of the Structure of Human Behavior

Glendale, California: Summer Institute of Linguistics, 1954. (Part I. Preliminary Edition.) Pp. 170.

By GEORGE A. MILLER

Harvard University

THE REASONING behind Professor Pike's most recent book might be summarized this way: All languages are human behavior. Linguists can analyze the structure of languages. Therefore, linguists can analyze the structure of all human behavior. The flaw in this syllogism is not difficult to find, but the fact that the conclusion does not follow necessarily from the premises is no guarantee that the conclusion is false. The syllogism could be saved, for example, by replacing the initial premise by its inverse, all human behavior is language. Pike does not go quite so far, but the implication is lurking behind the argument.

It is quite interesting to observe how a distinguished linguist goes about the job of extending the methodological tools developed for the analysis of language to the broader fields of nonverbal behavior. In order to develop "a unified theory of verbal and nonverbal human behavior," the first step is to isolate units of behavior. To this end, Pike examines the linguistic method in great detail; the "etic" approach, which classifies systematically all comparable data from all cultures of the world, is contrasted with the "emic" approach, which is an attempt to discover the pattern of behavior within a particular culture. The words "etic" and "emic" are taken over from the suffixes on the words "phonetic" and "phonemic." A segment of purposive human activity is called a "beahvioreme" and a verbal behavioreme is called an "uttereme." Behavioremes are hierarchically organized just as the component elements of a language are organized into a larger system.

The analysis begins with a unit of behavior such as a church service, a football game, or a family breakfast. This large unit has a structure such that certain spots permit the substitution of various alternative elements, just as the structure of a sentence permits the substitution of various alternative words at several spots. For example, during the family-breakfast behavioreme there comes a spot where the main course is served. Either a cereal-eating motif or a bacon-and-eggs-eating motif can fill that spot. In the more traditional terminology of linguistics, Pike seems to be saying that cereal and eggs are allophones of the same phoneme.

Linguistic analysis is a highly developed science and psychologists should be grateful to Pike for an example of how the techniques might be applied to a general analysis of behavior. The results would, however, have been more useful psychologically if Pike had been more familiar with modern psychology or had had advice from a good behavior theorist. He seems to have the idea that psychology is concerned only with matters of the mind and he prefers to stay with more objective, behavioral data. Most psychologists will probably give up in despair when they tangle with the elaborate terminology that Pike has created to express his ideas. If some courageous psy-

chologist nevertheless did persevere through the strange symbology and the unfamiliar approach, it is likely that he would come up with some good systematic ideas. Let us hope that there will be some worthy psychologist with the patience eventually to produce a readable translation. The present product is not yet suitable for psychological consumption.



Idiography Revisited

Jean Evans

Three Men

New York: Alfred A. Knopf, 1954.
Pp. xviii + 297. \$3.75.

By FRANK BARRON

University of California

THIS book is subtitled "An experiment in the biography of emotion." It represents itself as a true account of the lives of three men, whose stories are told not as an entertainment but as an exhibition of a new technique in psychological reporting. The author is a professional writer who has received financial support from philanthropic foundations for experimentation in the presentation of case histories, and the book itself was written while she was a Guggenheim Fellow in 1950. Of the three case studies which are presented, two have previously appeared as articles in the *Journal of Abnormal and Social Psychology*.

As a job of writing, *Three Men* is very well done. The author's style is fluid and impressionistic, and factual material from public records is interwoven with direct quotations from interviews and from the subjects' letters to give an unusual degree of convincingness to the presentations. The men whose lives are presented were themselves quite unusual individuals, and their stories, no matter how well or poorly told, would contain vivid and arresting material that could not fail to have considerable dramatic impact.

The first of them, Johnny Rocco, is a tough slum kid (or, rather, a deprived child who had either to toughen up or to give up). At age five he woke from sleep to see his slain father being carried bloody from the house, killed in a drunken brawl with his best friend. The second case

subject, William Miller, is a man blind and in prison, and the third, Martin Beardson, is a homosexual who finds life and himself disgusting, and who, as he looks at his own image in a mirror, sees himself as "unutterably old," though he is only twenty-five. Miller especially is a quite fantastic person who in his search for the meaning of life and death had descended into graves to have commerce with corpses and who had killed pregnant animals to study the formation of the foetus in the still-warm womb.

WITH extraordinary natural subjects for her portraiture, and with her own experience and talent as a journalist to aid her, it is not surprising that the author has written a compellingly interesting book. Its claim upon the special attention of psychologists, however, must rest, it seems to me, not upon its dramatic appeal or upon its demonstration that a case history can be well written, but upon the novelty and aptness of its technique and upon its heuristic power as a methodological device. On both counts I felt that the book claimed much more than it delivered. Gordon Allport writes in the Introduction to these case studies that "since they exemplify the blended (literary and scientific) approach to the study of personality, they will open new horizons to all students of human nature, whether their own preference leans toward the scientific or toward the literary approach." For myself, I find nothing in these case reports which would warrant the use of the word *scientific* in reference to them; they are a piece of journalism, and they have most of the merits and limitations one finds in coherent descriptions by a reporter of some psychologically interesting event which occurs only once. Nor do I find consistently in these reports the level glance at life which characterizes great literature (and even occasionally turns up in psychological formulations in a psychiatric clinic). The author has a tendency to sob somewhat over the plight of slum product Johnny Rocco, and to be disdainful of the "addled concepts" of homosexual Martin Beardson. I should say that one has a right to expect something more of both literature and science than one finds in this professed blend of the two.



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Personality Development in Adolescent Girls

New Orleans: Child Development Publications, Society for Research in Child Development, Inc., 1953. (Volume XVI, Serial No. 53.) Pp. 316.

By GRACE M. HEIDER

The Menninger Foundation

THIS monograph on personality development in adolescent girls is a preliminary report of a study whose detailed findings are to be presented in a series of publications by its individual authors. The over-all purpose of the study was (a) to explore the personality and emotional behavior of girls at three stages of development, and (b) to discover what each of several projective methods can contribute to such an investigation by using them on the same subjects. There were 300 subjects equally divided among three groups; (a) pre-pubescent girls, ages 10:6 to 14:6, (b) pubescent girls, ages 10:6 to 15:6, and (c) adolescent girls, ages 17:6 to 19:11. They were taken, with no other basis of selection than age and level of maturation, from nine different schools and colleges representing a wide range of social-economic, racial, and cultural backgrounds.

The main body of the work consists of five independently prepared chapters by four different investigators, each chapter presenting findings from one of the projective methods that was used,

including the Thematic Apperception Test, the Horn-Hellersberg Test, a test involving drawings of the human figure, the Rorschach test, and a graphological study. Sample records from individuals at each developmental level and an interpretive summary with suggestions for schools and agencies supplement these presentations.

The authors' general conclusions are that the study gives evidence of more frequent and more severe emotional disturbance than was anticipated. It is suggested that the schools are making demands for sustained study and academic achievement at an age when the psychological cost of meeting these demands is too great. It is urged that they should, on the one hand, provide curricula that would do more to aid their students to gain human understanding and to clarify their own feminine role in modern society, and, on the other hand, that they should use group tests to locate girls who show signs of impending disturbance and offer programs of group therapy to those who may need it.

Questions that are important for a consideration of the problems of adolescence are raised by this work. At the same time the findings from the different projective methods have not been brought into line with each other and variables of developmental status have not been adequately distinguished from those of background and individual endowment in the individuals that were studied. This makes it difficult to evaluate the presentation. One wonders whether the authors' own warning against the direct interpretation of projective data obtained from one age group in terms of norms derived from another (p. 22) has been sufficiently taken into account in the discussions of the findings on which the conclusions are based.



J'ai voulu être court. Il y a un grand secret pour être court: c'est d'être clair.

—M. J. P. FLOURENS



Special Classes

J. E. Wallace Wallin

Education of Mentally Handicapped Children

New York: Harper & Brothers, 1955. Pp. 485. \$4.50.

By D. A. WORCESTER

University of Nebraska

THIS book not only includes the substance of several other books written by Wallin over a period of many years, but in some ways it is a sort of autobiography of the writer's professional life. He was one of the first to advocate special facilities for mentally handicapped children in connection with the public schools, and his book provides one of the best overviews of the development of such classes during the last fifty years. One might fear that a person of Wallin's age would find his main interests in the work of some time past, but his references, which are numerous, include many in the 1950's and some that must have been included at the last moment before the manuscript went to press.

The discussion of the book includes all phases of the problem. Its historical section is concise and excellent. A good deal of space is given to the organization and administration of special classes—somewhat less to teaching procedures, qualifications of special class teachers, the curriculum, the problem of taking care of the mentally handicapped child in the regular grade, and so on.

The very scope of the treatment tends to reduce its effectiveness as a textbook for any particular purpose. The classroom teacher finds in it much material that is of no particular concern to her. There are some excellent suggestions on administration, but it would not be a book especially for administrators. This book was one of two texts used during the summer session of 1955 in a class on the education of the mentally handicapped. Almost unanimously, the class voted the other text as being the most helpful to them in terms of practical day-by-day suggestions, both for curriculum and teaching procedures. Many of them felt too that Wallin's language and style were not easy for the average student. There was agreement, however,

that this text is invaluable as a reference book. Information on almost any point concerning the topic can be located either in the text itself or in one of the references supplied. Occasionally, Wallin becomes a bit 'preachy,' sometimes somewhat repetitive, and occasionally, as in the chapter on the education of the mentally handicapped in the regular grades, he does not stick quite to his topic.

Speaking generally, this is a very valuable volume which should be on the reference shelf of every person working with the mentally handicapped.

Sociality: Whence and How

D. W. Harding

Social Psychology and Individual Values

London: Hutchinson's University Library, 1953. Pp. 184. \$2.40.

By DANIEL KATZ

University of Michigan

PSYCHOLOGY is perhaps the most self-conscious of all sciences in its attempts to be truly scientific. Its efforts are generally applied to a search for such formal models as seem to provide the mathematical precision of the more advanced sciences. Harding's thoughtful volume can, however, furnish us with a self-appraisal from another point of view, telling us how far our research and theorizing give us a valid account of the phenomena with which our science supposedly deals. With the historical perspective and the wisdom of an English scholar, Harding takes the position that "social psychology has not yet acquired any valid claim to unintelligibility, and it is still malleable enough to benefit from the attention and the questions of educated men and women." His brief treatise is a perceptive presentation of a number of problems which have not yet been adequately explored in social psychology.

The central question that Harding raises concerns the nature of sociality in human beings. He rightly puts his finger on the weakness of the derivation theories which assume that social drives develop out of more basic biological appetites, and he demonstrates the circularity of the Freudian explanation

that the basis of sociality lies in goal-inhibited libidinal impulses. His own preference is to view social desire as arising directly from an innate disposition. He fails, however, to consider the interaction theories of Mead and others who account for sharedness and sociality in their descriptions of the development of the self or the ego.

The questions that interest Harding center more about the individual than the collective aspects of social psychology. He punctures some of the existing confusion concerning concepts of normality and abnormality and gets us off to a good beginning for work in this field. His discussion of individual differences and their social significance is original. His essay on individual morality is reminiscent of Holt's *Freudian Wish* though, curiously for an English author, he takes its doctrine from a secondary source, the *Creative Experience* of Follett, a student of Holt's.

The chapters on the collective and interactional aspects of psychology are not so strong as the others, for the author is neither as well versed nor as interested in social class and status, in competition and leadership. Nonetheless he has done a service for social psychology in his insightful discussion of some of the problems which present-day psychologists usually ignore. It would be a fine contribution to the field if an equally wise and contemplative scholar were to undertake a similar examination of the variables of social process and social structure with which the social psychologist must eventually deal.

(continued from page 13)

encies. It can not do the impossible. It is limited to 384 pages per annum including advertisements. And you, gentle reader, have not paid for all your copy until you have bought some of the items advertised in *CP*. So identify with *CP*, if you can. Tell *CP* what you want changed, what you want done, and in extreme cases what you like about *CP*. Suggest reviews. Suggest reviewers. Suggest features. Send in apt quotations to put in embarrassing white spots at the bottoms of columns. You can own *CP* more than any small stockholder owns his company if you can but be moved to think for it and to express your thinking.

—E. G. B.

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Drug Addiction

Edward Podolsky

Management of Addictions

New York: Philosophical Library,
1955. Pp. 413. \$12.00.

By MERRILL MOORE

Boston, Massachusetts

This is a big book packed with information and wisdom. From his broad experience in the Department of Psychiatry, Kings County Hospital, Brooklyn, and general practice over many years, Edward Podolsky, M.D., has brought together most important and up-to-date opinions on the problems of addictions and their management.

The first part of this book deals entirely with alcohol, which is the number one addiction problem in the United States. It consists of twenty-six chapters, each written by prominent authors and it deals with all phases of this subject. It would make a fine book in itself. The second part of this book deals with drug addiction. In it are nine chapters reporting the findings and opinions of some of the most important scientists in American medicine.

The viewpoint of this book is broad and the attitude is inclusive and eclectic. It does not savor of partisan attitudes or 'schools.' Although many authors have combined to make this volume actually a symposium, there is a surprising homogeneity in the attitudes of the contributors. There has been much need for a book of this type and even though it leaves some difficult problems unsolved, it certainly approaches possible solutions in a constructive and progressive way. Such a book could not have been written fifty years ago because at that time medicine and psychiatry were not close enough together to afford the kind of unity this volume contains.

The volume is also provocative in some of its theoretical considerations and is packed with practical ideas and detailed descriptions of methods and syndromes. Especially helpful are the lists of references at the end of each chapter. This book will have wide value for physicians, psychologists, psychiatrists, and research workers in this and related fields.

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PSYCHOSOMATICS

By MAX HAMILTON, University of Leeds School of Medicine. An up-to-date, comprehensive introduction written in nontechnical language. The book is broad enough in scope and detailed enough in presentation to be useful to the research worker. The author presents

psychosomatics as a direct development of modern biological theory and demonstrates the necessity of looking beyond the assumptions of simple mechanisms in illness. The work includes a careful and detailed evaluation of the literature. 1955. 225 pages. \$4.25.

MEASUREMENT AND EVALUATION IN PSYCHOLOGY AND EDUCATION

By ROBERT L. THORNDIKE and ELIZABETH HAGEN, both of Columbia University. A comprehensive review of the concepts that underlie tests and measurement, and a guide to their application. The authors stress the objectives of testing rather than the mechanics. The book

includes a guide to the location of specific tests and gives a critical appraisal of the major methods of evaluating intelligence, aptitudes, achievements, and personality. 1955. 375 pages. Illus. \$5.50.

STOCHASTIC MODELS FOR LEARNING

By ROBERT R. BUSH and FREDERICK MOSTELLER, Harvard University. A unified description of the ways in which stochastic processes can be used to meet problems in the field of learning experiment. The stress is on learn-

ing and on the philosophy that learning is a probabilistic matter with events decreasing or increasing the probability of specific responses. 1955. 365 pages. \$9.00.

THEORIES OF PERCEPTION AND THE CONCEPT OF STRUCTURE

By FLOYD H. ALLPORT, Syracuse University. This unique, dual-purpose work begins with a searching examination of theories and findings in the field of perception in the light of logic and scientific method. It then presents a synthesis of the eight major generaliza-

tions more or less common to all the theories on perception and utilizes this in presenting a foundation for future systematic work in the form of a general theory of structure in behavior. 1955. 709 pages. \$8.00.

AN INTRODUCTION TO PSYCHOLOGY*

By HARRY W. KARN, Carnegie Institute of Technology; and JOSEPH WEITZ, Life Insurance Agency Management Association. A practical introduction to psychology for the non-specialist which gives the subject meaning in

terms of the reader's own experience without excluding the spirit and substance of the subject. Technical jargon and controversial positions are avoided and the work is as self-contained as possible. 1955. 315 pages. \$3.90.

PSYCHOLOGICAL STATISTICS, Second Edition*

By QUINN McNEMAR, Stanford University. In this revised and enlarged edition of a widely used book, the elementary treatment of statistical inference has been expanded to make the book more useful to the reader whose principal interest in statistics is as a tool. Offering

a balanced discussion of the statistical techniques most often used in psychological research, the work stresses assumptions and interpretations rather than routine computational procedures. 1955. 408 pages. \$6.00.

*One of the Wiley Publications in Psychology, Herbert S. Langfeld, Advisory Editor.

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8. THOMAS O'CONNOR AND JEROME H. ROTHSTEIN. An annotated directory of audio-visual aids dealing with the handicapped. (Mimeographed) San Francisco: San Francisco, Spec. Educ. Dept., 1951. Pp. ii + 53.

CP reports below on books about films, on research on the use of films and on films about Child Psychology. Next month CP will continue with reports on films about Group Living, Shop Safety, Emotional Behavior, and Perception, and also on some Recordings that are useful in teaching and in general education.

Books About Films

Edgar Dale

Audio-Visual Methods in Teaching (Rev. Ed.)

New York: Dryden Press, 1954. Pp. xiii + 534. \$6.25.

THE VARIETY of audio-visual materials available, and the increased use of such materials in teaching require systematization in a handbook, as in the book considered here. This

FILMS

By ADOLPH MANOIL, Editor

C P'S FILM DEPARTMENT covers productions for all fields of psychology—films, film strips, recordings, film research, and books. The department deals mainly, however, with films and writings about films. Many films are associated with books or supplemented by film strips or sound-film strips, thus extending the coverage to recordings which are included even when not directly related to a film or a film strip.

Now just what is a *psychological film*? Camera work has been used successfully as a technique of observation which allows accurate recording under special conditions of light, color, or speed, and of events otherwise difficult or impossible to describe. The use of a camera reduces the need and scope of inferential judgments by increasing repeatability, facilitating the accuracy of observation, and providing optimum conditions for comparative judgment. Through such techniques as high-speed photography, slow motion, time-lapse photography, or phase-contrast cinemicrography, a detailed study of events impossible to record and observe accurately in any other way becomes possible. Cinematographic studies, such as those on the startle pattern, the reflex behavior of infants, the vocal cords, plant behavior, meiosis and mitosis, mental health, and child psychology, are only a few examples of the new medium. Besides being a research tool, cinematography is also an art and a teaching device. As such it covers the whole area of behavior at all levels. Such films as *The Snake Pit*, *The Lost Weekend*, and *Gentlemen's Agreement* are examples of popular movies with definite psychological content. At a slightly different level we have the whole area of the 16-mm. educational film, which by definition, as a teaching and educational tool, should represent application of psychological principles.

The great variety of film production necessarily faces this department with a problem of definition. The definition of a

film as psychological, however, presents certain difficulties, for every film viewed in its relation to the spectator becomes a situation having psychological dimensions. Under these conditions, what is being considered is not the film in its objective aspects, but the relationship between the film and the viewer. The problem is whether to define a psychological film exclusively in respect of its content and intent or in respect of the use being made of it. For instance, there is a 16-mm. film called *Feeling All Right* (Mississippi State Board of Health, 1948, 30 min.). The content is a simple presentation of the conditions of a Southern town and the difficulty of the health authorities to make people accept syphilitic treatment. The main intent of the film is to communicate information about syphilis and so to educate for health. If, however, the film is viewed from the point of view of the principal character who shows various emotional responses, prejudice, etc., it becomes a particularly interesting psychological film, one that could lead to discussion on learning, prejudice, cultural problems, etc. Thus the definition of a psychological film becomes a matter of emphasis, either in terms of content or intent of the producer, or in terms of set and interpretation by the viewer. Still CP needs a practical working definition. Here is one.

Definition. A psychological film for CP's purpose is any film that explicitly or implicitly presents a situation of definite psychological relevancy. A situation having psychological relevancy is any situation that can be related, derived, or in any way meaningfully associated with a psychological principle, theory, or fact. This is the definition that will guide CP in the selection of films to be reviewed in this department.





volume describes systematically the whole audio-visual field as a methodological tool, a communication technique for teaching. In the learning process, the reality to be manipulated, transformed, and assimilated appears in various degrees between the concreteness of direct, immediate participation, and the abstractness of symbols or signs.

Through the use of a figure that he calls the "cone of experience," the author classifies experiential situations related to audio-visual materials, as extending from direct, purposeful experiences to verbal symbols. The intermediate stages are contrived experiences, dramatized experiences, demonstrations, field trips, exhibits, television, motion pictures, recordings-radio-still pictures, and visual symbols. This "cone of experience" as a visual device for presenting the audio-visual materials and their relative position in the learning process can be viewed as an integrated concrete table of contents of the book, and the

theory of audio-visual instruction (Part I) and classroom application of audio-visual methods (Part II) as adequate annotations. To avoid any misconception as to the meaning of this cone the author says: "The cone classifies sensory aids in terms of greater or less concreteness or abstractness as learning experiences, and *not with respect to the difficulty of the ideas they communicate*" (p. 54).

The presentation of the material is adequate and the book could be used with effectiveness for courses in the education of teachers and for general information. Each chapter is provided with references on books, special studies, audio-visual materials, and references to sources for these materials. The book is excellently printed, has a wealth of pictorial material, and a considerable number of color plates (12 of them full-page). A 14-page index-glossary makes for easy reference. More emphasis on research results and on the need for scientific evaluation of audio-visual materials would increase the volume's value.

Adolph Nichtenhauser, Marie L. Coleman, and David S. Ruhe

Films in Psychiatry, Psychology, and Mental Health

New York: Medical Audio-Visual Institute of the Association of American Medical Colleges. Health Education Council, 1953. Pp. 269. \$6.00.

THE WIDE use of films for teaching, training, and general education makes necessary their critical evaluation. Film evaluation represents a difficult task since any film, besides being a work of art, is at the same time a communicator of ideas and an educational tool. Proper evaluation requires an analysis not only in terms of the objective aspects of the film but also and mostly in terms of audience response. While an expert can judge a film from the point of view of his own technical competency, he can not presume to assess audience response without actual research.

The book being reviewed represents a remarkable contribution, especially at the level of expert judgment on the technical and communication value of films. It does not present research results on audience response, although the senior author explicitly states the desirability of such research (p. 24). With this exception the book provides thorough critical reviews of 51 films, and short descriptions of 50 others. For each of the 51 films reviewed, the book gives a short synopsis, suggested audience, production credits, availability, a detailed content description, and a critical appraisal as to content, presentation, effectiveness, and utilization. Each review represents a study in itself by one or more panels of experts.

To take one example only, in the review of *Out of True* it is said: "More than half of the story purports to show that Molly is cured by hospital treatment, yet her final improvement leading to discharge comes about only after the mother-in-law tells her she has decided to live elsewhere. The importance of this external change in Molly's family life tends to negate the central message of the film" (p. 171). What is the central message of this film? Is it the availability of psychiatric treatment, the importance

of education as to possible cure of mental abnormalities, the value of accepting treatment for mental illness as for any other illness, the importance of the social worker, the cooperation between family and the hospital, the value of the home environment. . . ? Besides, the film starts with the assumption that the main factor in Molly's difficulties is her mother-in-law. In the view of all these possibilities the film can be said to show no cure by hospital treatment only. The fact that the mother-in-law is leaving is not unimportant. There is, however, no 'central message of the film' except by the decision of the experts in terms of their main professional orientation.

Concerning the effectiveness of the film the text states: "Although it may dispel fears about institutional cure, its unflattering portrayal of the mother-in-law as a crucial factor in the onset of Molly's mental illness may evoke hostility in audiences of older people. . ." (p. 173). Now this might be true or not depending on the individuals in the audience. But besides this asserted value the question arises as to whether the mother-in-law is necessarily a "crucial" factor. Why should she not be simply a contributing or precipitating factor, even on the assumption of the "unconscious equation of the mother-in-law with her own mother". One might indeed question the very "psychodynamic message," as the only and exclusive explanatory concept, if such were really the intent of the film. Critical appraisal should consider the teaching or educational value of the film not only with reference to what is presented but also to what is omitted, as the authors have done in the review of Gesell's film on *The Embryology of Human Behavior*.

The title of the book would have been more appropriate if the word psychology had been replaced by abnormal psychology, since no films on experimental, industrial, animal, physiological, or social psychology are presented.

The book can be used as a dependable source for the selection of films and also as a guide-commentary to supplement the showing of films. It also provides useful information on film reviewing techniques, the use of films in the teaching of psychiatry and a historical précis on the use of films in education in neurology, psychiatry, psychology and mental health.

Eight pages of stills from various films supplement the text.

Film Research

Kenneth E. Anderson, Fred S. Montgomery, Herbert A. Smith, and Dorothy Smith Anderson (School of Education, University of Kansas)

Toward a More Effective Use of Sound Motion Pictures in High School Biology

Paper presented at the 1954 Chicago Convention of NARST (National Association for Research in Science Teaching).

THE PAPER presents a research on the effectiveness of motion pictures in teaching. The study shows that better results are obtained when films are used with emphasis on the principles covered or stressed, and also when films are used regularly than when used not at all or only occasionally. The study is supplemented by a chart of the 20 films used, and principles stressed or covered in each. The report is a good example of needed research in the area of film utilization for teaching. The research demonstrates the importance of mental set and indicates the need for a classification of films in terms of specific content analysis. Obviously there is a need for screening and adequate preparation when films are to be used in teaching.

Donald M. Johnson and Walter G. Vogtman (Michigan State College)

A Motion Picture Test of Achievement in Psychology

American Psychologist, 1955, 2, 69-71.

THE USE of motion pictures for achievement tests in psychology is shown to be feasible. The authors have used the film *Of Skates and Elephants*, 20 min. and a test of 28 five-choice items. The results in this study would indicate very little achievement as the result of a course in beginning psychology. "Aside from terminology psychologists have nothing to teach them" (students). This statement, given as a possible implication of the study, certainly may not be generalized. The area needs further exploration with different types of items, different types of films, and different types of presentation.

Child Psychology

FLMS are one of the most appropriate means for the study of child development, as they allow for accurate and repeated observation. A great number of 16-mm. films on child development and child problems are now available. (See, e.g., Child Welfare Films. An international index of films and filmstrips on the health and welfare of children. Unesco, 1950. Pp. vi + 213.) Following are a few recent 16-mm. black-and-white sound films in this area.



Building Children's Personalities with Creative Dancing

Lawrence Frank and Gary Goldsmith. Black and white or color, 30 min., 1953. Available through Education Film Sales Dept., University Extension, University of California, color \$275.00; rental \$7.50; black and white \$135.00; rental \$5.00.

THE CHILD'S need for activity and expression, his need to belong and grow, his imitative tendencies and responses to a dynamogenic environment could be channelized through activities such as dancing, especially when opportunity for creativeness and initiative is provided. This film presents creative dancing as it affects the development of the child with due allowance for individual differences. A creative dancing class conducted by Mrs. Gertrude Knight is shown; the effect on children 5 to 10 years old is to be inferred through observations of their behavior. Dancing as an art affects the child in his growth by releasing tensions and facilitating free physical expression.

The film represents a lesson in dancing with a group of children, and it has definite artistic value. As to its value in demonstrating development of children's personalities further research would be necessary. The film suggests, however, the possibility of considering creative dancing as providing for insight into child development problems. In the field of child study this would put the area of creative dancing on the same level with music, drawing, or painting. The film could be used as a teaching tool under this perspective.

A Long Time to Grow: Part I, Two and Three-Year-Olds in Nursery School

Dept. of Child Study, Vassar College, 35 min., 1951. Available through New York University Film Library, 26 Washington Place, New York 3, N. Y. \$120.00; rental \$6.00.

TWO- AND three-year-olds are presented in their supervised and spontaneous behavior in the nursery school. Various film sequences show characteristic aspects of play activities, differential features between the two and the three-year-olds, imitative play, activity for its own sake, make-believe activities, finger and brush painting, use of nursery equipment, as well as characteristic outdoor play. Children are shown at lunch and in various other indoor situations.



GROWING UP: A NEW SKILL

(From the film *A Long Time to Grow: Part II*. New York University Film Library)

A Long Time to Grow: Part II, Four and Five-Year-Olds in Nursery School

Mary Fisher Langmuir, Eveline Omwake, Lawrence Joseph Stone; technical direction, Joseph Bohmer, Dept. of Child Study, Vassar College. 35 min., 1954. Available through New York University Film Library, \$120.00; rental \$6.00.

THIS second film presents in detail the behavior of the four and five-year-olds in nursery school. The activities portrayed through actual photography refer to (1) vigorous group play (barrel-rolling, jumping, use of clay, tree-climbing, swinging), (2) sen-

sory pleasure (music, soap bubbles, interest in color, shape, pattern, taste, smell, home play activities), (3) make-believe (imaginary mother and baby play, shooting, digging, outdoor blocks, painting), and (4) assurance with materials (painting, use of tools, workmanship). Other behavioral characteristics are (1) recognition of rules for the games, (2) interest in facts, and (3) use of symbols.

Both these films present characteristic activities of children as they occur in a nursery school. The importance of a permissive atmosphere with a minimum of supervision is stressed throughout the film. The role of the student teacher and training conditions are also shown.

The films could be used as training films with student teachers, as an exercise in observation of child behavior, or as a means to demonstrate various principles of child individual and social development. They could also be used with lay audiences and as a stimulant for discussion. The script, which is provided with the second film, is a useful supplement for teaching. For research purposes and training of observers, the film could be used with or without sound track; this would allow for observation and interpretation without the suggestive comments of the narrator.

Maternal Deprivation

J. Aubry and Geneviève Appell; technical direction, P. P. Gonse and J. J. Petter. 30 min., 1953. Available through the New York University Film Library.

THE EFFECT of maternal deprivation on the general behavior of the child is clearly illustrated by the spontaneous activities of children in a nursery. The first part of the film shows the apathy, lack of contact with the adult, stereotyped movements, and general withdrawal of children who have been separated from their mothers for different periods of time. The second part shows the rehabilitation of these children as a result of psychotherapy. A few cases are shown in detail before and after therapy.

This film is a part of a series of studies on deprivation of maternal care made by the Centre International de l'Enfance,

Paris, France. The film is provided with a systematic guide including references; it could be used with classes in child psychology, and lay audiences. (See also the comments on the film *A Two-Year-Old Goes to the Hospital*).

A Two-Year-Old Goes to the Hospital

James Robertson. Tavistock Clinic, London, 45 min., 1953. Available through the New York University Film Library.

THIS is a research film presenting without interpretation the behavior of Laura, 2 years 5 months old, during eight days without her mother (except at visiting hours) in a hospital for a slight operation. The film was taken daily at the same hour and under similar conditions so as to provide behavior samples, independent of photographer's time, predilections, and other subjective factors.

Two problems are raised by this film: (1) the effect of mother deprivation on the young child, and (2) the hospital position toward recognition of the effect of mother deprivation. Concerning the first problem the film shows withdrawing effects, apathy, gradual indifference, and slight anxiety; also characteristic reactions to the hospital environment, anesthesia, and surgery. Laura's attachment to certain personal possessions (a Teddy bear, a blanket) and her rough handling of a hospital doll are also seen.

The film was made as part of a research at the Tavistock Clinic, London. Its hypothesis is "that a pre-requisite of mental health is the experience of a warm intimate and continuous relationship to the mother in the early years of life—or in the absence of the mother to one other person." The immediate problem is to discover "how much loss of maternal care the young child can bear without harm." The film, as a behavior sample of Laura, is adequate, and its research value is increased by a comprehensive guide (with bibliography) which gives information on Laura's history before the hospital period and after.

The film can be used with or without sound track either in classes in child psychology or with lay audiences, when appropriate leadership is available.

ON THE OTHER HAND...

This is the department of Letters to *CP*, the department for dissents and at times for the bright ideas of *CP*'s readers about books, publication, and *CP*. Here readers can criticize *CP* policy or, if there is any space left, praise it even. Here authors may on occasion criticize their critics or substitute what they think is truth for what they think is fiction, provided, of course, that their emotions can be confined to terse expression.

Convention ordinarily interdicts an author from replying to a criticism of his book, no matter how unfair or untrue the review may seem to him. That rule appears to compound unfairnesses, and *CP* believes that good criticism cannot be divorced from discussion. The critic's review, be it fair or foul, is inevitably diosyncratic. It cannot dispense final

truth, as if the critic were superhuman. At best it can impart some information and arouse some thought. Criticism is a form of autobiography. (Did Oscar Wilde say that?) Neither critic nor author is infallible, nor should either be considered inviolable.

On the other hand, the author, always impeded by his own ego involvement, is usually not his own best defender. If a critic is to be corrected or rebuked, *CP* would prefer to have the admonishment administered by some reader other than the author, by some one eager to right a wrong, by a friend of the author's, perhaps, but in any case by a psychologist whose amour propre has not been wounded and who might therefore escape the egoistic bias. But *CP* means to be fair and not to muzzle the critics nor the critics' critics.

Nevertheless, complete freedom for critical expression is impossible. A series of rejoinders must converge rapidly when journal space is as limited as it is today. *CP* at its start boldly takes a stand for freedom of criticism. Can it keep this up, or will it presently be censoring the critics' critics under the familiar warrant of lack of space? It is too soon to say.

There may be some readers who believe simply that a department of letters is not a good use for *CP*'s pages. Fortunately they will have a Department of Letters in which to dislike a department of letters.

For obvious reasons *CP* has to begin its ON THE OTHER HAND... without any letters at all. These paragraphs are but the prologue before the play, and an invitation. Readers and authors awake! You have an open (if narrow) channel for communication. What thoughts proper to *CP* have you to communicate?

—E. G. B.

Books Received

Through 30 September 1955: A-H

ABRAMSON, H. A. (Ed.). *Neuropharmacology*. (Transactions of the First Conference, May 26-28, 1954.) New York: Josiah Macy, Jr. Foundation, 1955. Pp. 210. \$4.25.

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